

# ANALYSIS

Edited by Margaret Macdonald, with the  
advice of A. J. Ayer, R. B. Braithwaite,  
Herbert Dingle, A. E. Duncan-Jones, C. A.  
Mace, A. M. MacIver, and H. H. Price

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LUDWIG WITTGENSTEIN<sup>1</sup>

By GILBERT RYLE

AN original and powerful philosopher, Ludwig Wittgenstein, an Austrian who finally became a naturalized British subject, came to England shortly before the first World War to study engineering. In 1912, bitten by logical and philosophical problems about the nature of mathematics, he migrated to Cambridge to work with Bertrand Russell. During that war, he was in the Austrian army and ended up a prisoner of war. In this period he wrote his one book, the famous "Tractatus Logico-Philosophicus", of which a not quite reliable English translation was published in 1922. He taught in an Austrian village school for some time, during which he came into close philosophical touch with a few of the leading members of the Vienna Circle. In 1929 he came to Cambridge, where the importance of his ideas had been quickly recognized. In 1939 he became Professor. For part of the last war he was a hospital orderly at Guy's Hospital. In 1947 he resigned his Chair. Besides the "Tractatus", he published only one article.

In the last twenty years, so far as I know, he published nothing; attended no philosophical conferences; gave no lectures outside Cambridge; corresponded on philosophical subjects with nobody and discouraged the circulation even of notes of his Cambridge lectures and discussions. But with his serious students and a few colleagues, economists, mathematicians, physicists and philosophers, he would discuss philosophical matters unwearingly. Yet from his jealously preserved little pond, there have spread waves over the philosophical thinking of much of the English speaking world. Philosophers who never met him—and few of us did meet him—can be heard talking philosophy in his tones of voice; and students who can barely spell his name now wrinkle up their noses at things which had a bad smell for him. So what is the difference that he has made to philosophy?

It is vain to try to forecast the verdict of history upon a contemporary. I have to try to do this for one who has for about 30 years avoided any publication of his ideas. So what I offer is a set of impressions, interpretations, partly, of mere echoes of echoes.

From the time of Locke to that of Bradley philosophers had debated their issues as if they were psychological issues. Cer-

<sup>1</sup> A B.B.C. Third Programme talk, given on May 26, 1951.

tainly their problems were, often, genuine philosophical problems, but they discussed them in psychological terms. And if they asked themselves, as they seldom did ask, what they were investigating, they tended to say that they were investigating the workings of the mind, just as physical scientists investigate the working of bodies. The sorts of "Mental Science" that they talked were sometimes positivistic, sometimes idealistic, according, roughly, as they were more impressed by chemistry than by theology or *vice versa*.

However, fifty years ago philosophers were getting their feet out of these psychological boots. For psychology had now begun to be done in laboratories and clinics, so arm-chair psychology became suspect. But even more influential was the fact that logical quandaries had recently been exposed at the very roots of pure mathematics. The mathematicians needed lifelines, which they could not provide for themselves. Logicians had to work out the logic of mathematics, and they could not base this logic on the findings of any empirical science, especially of so hazy a science as physchology. If logic and philosophy were not psychological enquiries, what were they?

During the first twenty years of this century, many philosophers gave another answer to this question, a Platonic answer. Philosophy studies not the workings of minds or, of course, of bodies either; it studies the denizens of a third domain, the domain of abstract, or conceptual entities, of possibilities, essences, timelessly subsisting universals, numbers, truths, falsities, values and meanings. This idea enabled its holders to continue to say that philosophy was the science of something, while denying that it was the science of any ordinary subject-matter; to champion its autonomy as a discipline, while denying that it was just one science among others; to give it the standing of a science while admitting its unlikeness to the sciences. Thus the question "What are philosophy and logic the sciences of?" received a new answer, though one with a disquietingly dreamlike ring. It was the answer given by Frege and by Russell.

In Vienna thinkers were facing much the same question, though from an opposite angle. Whereas here it had been widely assumed that philosophy was Mental Science, and therefore just a sister-science to physics, chemistry, zoology, etc., in the German speaking world it was widely assumed that philosophy stood to the other sciences not as sister but as mother—or even governess. Somehow professors of philosophy there enjoyed such a pedagogic domination that they could

dictate even to the scientists. *Of course* philosophers were the right people to decide whether the teachings of Darwin, Freud and Einstein were true.

Late in the 19th century Mach had mutinied against this view that metaphysics was a governess-science. By the early 1920s this mutiny became a rebellion. The Vienna Circle repudiated the myth that the questions of physics, biology, psychology or mathematics can be decided by metaphysical considerations. Metaphysics is not a governess-science or a sister-science ; it is not a science at all. The classic case was that of Einstein's Relativity principle. The claims of professors of philosophy to refute this principle were baseless. Scientific questions are soluble only by scientific methods, and these are not the methods of philosophers.

Thus, in England the question was this. What are the special virtues which the natural and the mathematical sciences lack but logic and philosophy possess, such that these must be invoked when the former find themselves in quandaries ? In Vienna the question was this. Given that philosophers cannot decide scientific questions, what are the logical virtues which scientific procedures possess, but philosophical procedures lack ? The contrast between philosophy and science was drawn in both places. In Vienna, where the autonomy of the sciences was actually challenged the object was to expose the pretensions of philosophy as a governess-science. Here, where, save for psychology, the autonomy of the sciences was not seriously challenged, it was drawn in order to extract the positive functions of logic and philosophy. Philosophy was regarded in Vienna as a blood-sucking parasite ; in England as a medicinal leech.

To Wittgenstein the question came in its English form. And so he could not be called one of the Logical Positivists. Their polemics were not his ; and his quest for the positive function of logic and philosophy was not, until much later, theirs. He was influenced by Frege and Russell, not by Mach. He had not himself felt the dead hand of professorial philosophy which cramped, and still cramps, even scientific thought in Germany and Austria. He, conversely, himself helped to fix the logical lifelines for the mathematicians.

I want to show how Wittgenstein transformed and answered what was all the time his master-question, "What can philosophers and logicians do, and how should they do it ?"

I have said that after a long imprisonment in psychological idioms, philosophy was, for a time, re-housed in Platonic

idioms. But this was only a temporary asylum. For after a short period during which philosophers tried not to mind the dream-like character of the new asylum, something awoke them from the dream. Russell, in his enquiries into the logical principles underlying mathematics, found that he could not well help constructing statements which had the logically disturbing property that they were true only on condition that they were false, and false only on condition that they were true. Some of these self-subverting statements seemed to be inherent in the very basis which was to make mathematics secure. There was a major leak in the dry dock which Frege and he had built for mathematics.

Russell found a patch for the leak. Underlying the familiar distinction between truth and falsehood, there is a more radical distinction between significance and meaninglessness. True and false statements are both significant, but some forms of words, with the vocabulary and constructions of statements, are neither true nor false, but nonsensical—and nonsensical not for reasons of wording or of grammar, but for logical reasons. The self-subverting statements were of this sort, neither true nor false, but nonsensical simulacra of statements. Notice, it is only of such things as complex verbal expressions that we can ask whether they are significant or nonsense. The question could not be asked of mental processes ; or of Platonic entities. So logic is from the start concerned, not with these but rather with what can or cannot be significantly said. Its subject-matter is a linguistic one, though its tasks are not at all those of philology.

In Wittgenstein's "Tractatus" this departmental conclusion is generalised. All logic and all philosophy are enquiries into what makes it significant or nonsensical to say certain things. The sciences aim at saying what is true about the world ; philosophy aims at disclosing only the logic of what can be truly or even falsely said about the world. This is why philosophy is not a sister-science or a parent-science ; that its business is not to add to the number of scientific statements, but to disclose their logic.

Wittgenstein begins by considering how a sentence, a map, a diagram or a scale-model can represent or even significantly misrepresent the facts. The isolated words 'London' and 'south' are not true or false. Nor can a single dot on a sheet of paper be an accurate or inaccurate map. The sentence 'London is north of Brighton' is true. The same words, differently arranged as 'Brighton is north of London' make a false state-

ment. Arranged as 'South is London of Brighton' they make a farrago which is neither true nor false, but nonsense. For dots on paper to represent or misrepresent the direction of Brighton from London, there must be a dot for each town and they must be set out in accordance with some convention for points of the compass. For a statement, map or diagram to be true or false, there must be a plurality of words or marks ; but, more, these bits must be put together in certain ways. And underlying the fact that the truth or falsity of the statement or map partly depends upon the particular way in which its bits are arranged, there lies the fact that whether a significant statement or map results at all, depends wholly on the general way in which the bits are put together. Some ways of jumbling them together are ruled out. What rules rule them out ?

In the "Tractatus" Wittgenstein came to the frustrating conclusion that these principles of arrangement inevitably baffle significant statement. To try to tell what makes the difference between significant and nonsensical talk is itself to cross the divide between significant and nonsensical talk. Philosophising can, indeed, open our eyes to these structural principles, but it cannot issue in significant statements of them. Philosophy is not a science ; it cannot yield theories or doctrines. None the less it can be skilful or unskilful, successful or unsuccessful. It is in pursuing the activity itself that we see what we need to see. Rather like learning music or tennis, learning philosophy does not result in our being able to tell what we have learnt ; though, as in music and tennis, we can show what we have learnt.

Now it is true that philosophical clarity is achieved in the acts of appreciating arguments rather than in propounding theorems. But it is false that all philosophical talk is nonsensical talk. Wittgenstein had himself said very effective things, and talking effectively is not talking nonsensically. What had brought him to this frustrating conclusion ? When he wrote the "Tractatus", he was, I think, over-influenced by his own analogies between saying things and making maps, diagrams and scale-models. Certainly, for marks on paper to constitute a temperature-chart, or for spoken words to constitute a significant statement, the dots and the words must be arranged according to rules and conventions. Only if the zigzag of dots on the nurse's graph-paper is systematically correlated with the thermometer-readings taken at successive moments of a day, can it represent or even misrepresent the alterations in the patient's temperature. Only if words are organized according

to a number of complex general rules does a true or false statement result.

Suppose we now asked the nurse to depict on a second sheet of graph-paper, not the course of the patient's temperature, but the rules for representing his temperature by dots on graph paper, she would be baffled. Nor can the rules and conventions of map-making themselves be mapped. So Wittgenstein argued in the "Tractatus" that the philosopher or logician is debarred from saying what it is that makes things said significant or nonsensical. He can show it, but not tell it. After the "Tractatus" he realised that though saying things does resemble depicting things or mapping things in the respect for which he originally drew the analogy, it does not resemble them in all respects. Just as the nurse can tell, though not depict, how the temperature-chart represents or misrepresents the patient's temperature, so the philosopher can tell why, say, a scientist's statement makes or does not make sense. What alone would be absurd would be a sentence which purported to convey a comment upon its own significance or meaninglessness.

The "Tractatus" has two distinct but connected aims. The first, which I have crudely sketched, is to show both what philosophy is not, namely any sort of a science, and what it is, namely an activity of exploring the internal logic of what is said, for example, in this or that scientific theory. The second, which I shall not even try to sketch, is to show what sort of an enquiry Formal Logic is. This brings me to a general point about the "Tractatus". Wittgenstein's first interest had been in the logic of mathematics and thence in the logical paradoxes which were the big leak in the dry dock that Frege and Russell had built. He was, therefore, equipped and predisposed to squeeze whatever can be significantly said into the few statement-patterns with which the logic of mathematical statements operates. He used its terminology, its codes, and its abacus-operations in his task of exploring various philosophical issues, and, above all, his own master-issue, that of the nature of philosophising itself. In consequence, the "Tractatus" is, in large measure, a closed book to those who lack this technical equipment. Few people can read it without feeling that something important is happening; but few experts, even, can say what is happening.

But this is not the end of the story. Maybe it is only the preface. For, after lying fallow for some years, Wittgenstein returned to philosophy. His teaching in this period differs markedly from that of the "Tractatus"; it even repudiates parts of the "Tractatus".

First, he no longer forces all expressions into the favoured few patterns of the logic of mathematics. With this goes a revolt against moulds of any sorts. The rubrics of logical systems and the abstract terms of philosophical schools are like the shoes of Chinese ladies, which deformed their feet and prevented them from walking on them. Philosophical elucidation is still inspection of expressions, but it is no longer inspection through the slots of a logician's stencil or through the prisms of a scholastic classification-system. His diction has reverted from that of a Russell discussing esoteric matters with mathematicians to that of a Socrates discussing everyday ideas with unindoctrinated young men. Nor does he now elucidate only the propositions of the sciences. Like Moore, he explores the logic of all the things that all of us say.

Next, though I think that his master-problem is still that of the nature, tasks and methods of the philosophical activity, he no longer thinks that philosophers are condemned to trying to say the unsayable. But he now avoids any general statement of the nature of philosophy, not because this would be to say the unsayable, but because it would be to say a scholastic and therefore an obscuring thing. In philosophy, generalisations are unclarifications. The nature of philosophy is to be taught by producing concrete specimens of it. As the medical student learns surgery by witnessing and practising operations on dead and on live subjects, so the student of philosophy learns what philosophy is by following and practising operations on particular quandary-generating ways of talking. Thus Wittgenstein would rove, apparently aimlessly because without any statement of aim, from one concrete puzzle to its brothers, its cousins, its parents and its associates, demonstrating both what makes them puzzling and how to resolve them—demonstrating, but not telling ; going through the moves, but not compiling a manual of them ; teaching a skill, not dictating a doctrine.

One favourite procedure of his might be called the " tea-tasting method ". Tea-tasters do not lump their samples into two or three comprehensive types. Rather they savour each sample and try to place it next door to its closest neighbours, and this not in respect of just one discriminable quality, but along the lengths of various lines of qualities. So Wittgenstein would exhibit the characteristic manner of working of a particular expression, by matching it against example after example of expressions progressively diverging from it in various respects and directions. He would show how striking similarities may go with important but ordinarily unremarked differences, and

how we are tempted to lean too heavily on their similarities and hence to be tripped up by their latent differences.

For philosophers do not examine expressions at random. The quest for their internal logic is forced upon us by the fact that we find ourselves already caught up in unforeseen entanglements. Why do we slide into quandaries? Let me invent an example. We find ourselves talking as if like a train, so time itself might one day slow down and stop. We divide a train into coaches and coaches into compartments. We divide a month into weeks and weeks into days. When a train is passing me, some coaches are beyond me, some are still to come, and one compartment of one coach is directly abreast of me. I look at its occupants through the window. Surely time is like this. Last week has gone, next week is still to come, but I can exchange glances with the occupants of Now. So, as trains always slow down and stop somewhere, what makes time puff on so tirelessly? Might not Now be the last compartment of the last coach? Yet surely not; there would still be something behind it, if only the empty wind. You see that it is tempting, but also that it smells like nonsense to speak of the last compartment of time. Why may we say some things about time which are very much like some things that we legitimately say about trains, when to some of the proper corollaries of what we say about trains there correspond no proper corollaries about time? To answer this question, we should have to examine the functioning of whole ranges of things that we say about trains, rivers and winds; about moving shadows, rainbows and reflections; about perpetual motion machines, stars, clocks, sundials, and calendars; about the series of numbers, days of the week and minutes of the day. And then we may see why we slid and no longer incline to slide from the proper corollaries of familiar dictions about trains to corresponding corollaries of somewhat similar dictions about time. We see that we had overpressed certain analogies between ways of talking; and that we were so dominated by a favourite model, that we had gone on using it where it could no longer work. And now we know, in a way, what time is, though there is no shorter or better way of saying what time is than by going through again the same sort of process of linguistic tea-tasting.

I must conclude. Wittgenstein has made our generation of philosophers self-conscious about philosophy itself. It is, of course, possible for a person to be very thoughtful about the nature and methods of an activity, without being made any the better at performing it. The centipede of the poem ran well

until he began to wonder how he ran. Maybe we have been made a bit neurotic about the nature of our calling. But Wittgenstein's demolition of the idea that philosophy is a sort of science has at least made us vigilant about our tools. We no longer try to use for our problems the methods of arguing which are the right ones for demonstrating theorems or establishing hypotheses. In particular we have learnt to pay deliberate attention to what can and cannot be said. What had, since the early days of this century, been the practice of G. E. Moore has received a rationale from Wittgenstein ; and I expect that when the curtain is lifted we shall also find that Wittgenstein's concrete methods have increased the power, scope and delicacy of the methods by which Moore has for so long explored in detail the internal logic of what we say.

*Magdalen College, Oxford.*

#### A NEW FORMULA FOR THE SYLLOGISM IN TERMS OF THE ORDINARY SENSE OF 'IMPLICATION'

By A. C. EWING

RECENT advances in symbolic logic have been effected largely by men who believed that the best way of treating arguments was extensional rather than intensional, but I am not convinced that this is more than a historical accident and that an at least equally satisfactory logic might not be built up on an intensional basis. Extension after all seems posterior to and dependent on intension rather than vice versa. Membership of a class depends on qualities and relational properties, and univeral propositions on connections of properties. So I shall try to give an intensional account of the syllogism which will strictly prove the validity of all the recognised figures, leaving it to someone who is more expert than I am in other branches of logic to apply similar principles there if he should deem it worth while. I shall not make use of "implication" in the Russell sense, which is essentially an extensional conception ; but shall return to what is still the normal meaning of the term outside formal logic, and treat this as my fundamental concept. In this sense of "imply"  $p$  implies  $q$  when and only when there is a special relation between  $p$  and  $q$  such as would entitle us to infer  $q$  from  $p$  in the event of  $p$  being true. This relation I shall signify by an arrow pointing in the direction of the proposition which is implied. I might have called it the relation of "entail-

ing", but although this would be again in accordance with ordinary usage, it might confuse those who limit "entailment" to cases of formal and logically necessary implication. I mean to cover not only those but all cases where we deem ourselves entitled to infer one proposition from another, whether because of formal logic, because of a supposed causal or other not formally necessary connection, or because of some authority which we accept. It is a pity that the word *implication*, which was and still is so generally used to express just this, has been by its diversion to another sense spoilt for formal logic. For a general term to express any relation justifying inference is needed by the logician.

Most modern logicians have treated universal propositions as not having existential import, but they have adopted a different line as regards particular propositions. I propose to give a non-existential interpretation of the latter also. It will not, of course, express their full meaning, but neither does a non-existential interpretation of universal propositions, and I think both express an important part of the meaning, though no complete logic should fail also to include the existential interpretations and point out the logical consequences which result when propositions are interpreted in the latter way. Now the non-existential element in the universal proposition all  $S$  is  $P$  seem to me to be adequately conveyed by the hypothetical: If  $x$  is  $S$ , then it is  $P$ . This is not equivalent to " $x$  is  $S$  implies" " $x$  is  $P$ " in the Russell sense, because we certainly do not regard as true all hypothetical propositions where the antecedent does not stand for a fact. Such a proposition may be true, but only if there is a special relation between antecedent and consequent such that, if the antecedent had been a true proposition, we should have been entitled to infer the consequent as true also.<sup>1</sup> This relation I have decided to express by  $\rightarrow$ , so all  $S$  is  $P$  becomes  $(x) \phi x \rightarrow \psi x$ , taking 'propositional functions' in the usual sense and using  $\phi x$  to stand for " $x$  is  $s$ " and  $\psi x$  for " $x$  is  $p$ ". But it will be more convenient if I am allowed to abbreviate still further and just substitute "s" for the function " $x$  is  $s$ ", etc. By " $s \rightarrow p$ " I shall mean that any true proposition formed from the function " $x$  is  $s$ " by making a substitution would be so related to the corresponding proposition formed from the function " $x$  is  $p$ ", that the latter could be inferred from the former. It will be an advantage in clarity of exposition to retain the letters  $s, p, m$  so familiar to

<sup>1</sup> I am not intending this as a *definition* of a hypothetical. If so, it would be circular.

students of the syllogism. I shall use a bar above a term to signify "not".

But how are particular propositions to be interpreted non-existentially? We are told that the particular is the contradictory of the universal. Well, what is the contradictory of  $s \rightarrow p$ ? Surely "p does not follow from s", or "s is compatible with  $\bar{p}$ ". So we have A—s→p, E—s→ $\bar{p}$  I—s=  $\bar{p}$ , O—s=  $\bar{p}$ . It may be objected that I have eviscerated particular propositions to such a point as to deprive them of all value, but it is very often of some considerable use to know that two things are compatible. A patient may very much want to know whether pursuing his ordinary occupation is compatible with his return to health or a jury whether the evidence is compatible with a prisoner's innocence. In science it is often a most important step to point out that two things were compatible which had been supposed incompatible, and many men have been very worried indeed as to whether freedom is compatible with causality or the goodness of God with the existence of evil. A discovery of consistency is indeed usually valued not for its own sake but rather as a preliminary to establishing further truths, but that this is the case with particular propositions has long been a commonplace in logic.

Now it is not possible in the space to demonstrate by my method or any other all recognised valid syllogistic moods, but this would also be unnecessary. They fall into three distinct groups, and if I demonstrate one or two of each of the three, it will be quite clear how the others also could be demonstrated by applying the same method. The first group is the one in which both premisses and conclusion are universal. The Barbara mood becomes:  $m \rightarrow p$ ,  $s \rightarrow m$ ,  $\therefore s \rightarrow p$ . This may be proved as follows:

Suppose s true for any particular value of x e.g. Socrates. Then m is true also for that value, and if m is true so is p.  $\therefore s \rightarrow p$ . There are of course other ways of proof. Obviously there would be no difference of principle if for p we substituted  $\bar{p}$ , but the argument would break down altogether if for  $s \rightarrow m$  we substituted  $s \rightarrow \bar{m}$  (rule that minor premiss of first figure must be affirmative), unless we substituted  $\bar{m}$  for m also in  $m \rightarrow p$ , in which case  $\bar{m}$  would be the middle term and the minor would under the old formulation be treated as affirmative. Second figure syllogisms with a universal conclusion can be demonstrated equally easily but not in quite the same way. Here  $s \rightarrow m$  and  $p \rightarrow m$  or vice versa. Therefore if both s and p were true of the same value of x, there would be a contradiction ( $\bar{m}\bar{m}$ ).  $\therefore s \rightarrow \bar{p}$  (sEp).

The second group consists of syllogisms in which one premiss is particular. Let us take the *third* figure syllogism—some M is not P, all M is S, some S is not P ( $= m \neg p, m \rightarrow s, \therefore s \neg p$ ). This may be proved as follows:

Suppose  $s \rightarrow p$ . Then since  $m \rightarrow s, m \rightarrow p$ , which contradicts the major. If we took, for instance, the mood AII in the first figure, the proof would be somewhat different. The mood runs— $m \rightarrow p, s \neg m, \therefore s \neg p$ . Proof: Suppose  $s \rightarrow \bar{p}$ . Now  $m \rightarrow p, \therefore \bar{p} \rightarrow \bar{m}, \therefore s$  would  $\rightarrow \bar{m}$ , which contradicts the minor premiss.

The third group consists of syllogisms in which there are two universal premisses and a particular conclusion, e.g.  $m \rightarrow p, m \rightarrow s, \therefore s \neg \bar{p}$ . This class of syllogism has been abandoned as invalid by modern logicians because they interpreted universal propositions non-existentially, thus rendering an existential conclusion impossible. Since I am treating both premisses and conclusion as non-existential, this does not apply to my account, and I can prove these also, thus. Suppose  $s$  did  $\rightarrow \bar{p}$ . Then, since  $m \rightarrow p$  and  $m \rightarrow s$ , we should have for some value  $c$  of  $x$ ,  $c$  is  $m$ ,  $c$  is  $p$ ,  $c$  is  $s$ , and also since *ex hypothesi*  $s \rightarrow \bar{p}$ ,  $c$  is  $\bar{p}$ , a contradiction. With this group I have however to make an assumption which in the case of the first two groups I did not have to make, namely that  $m$  (or, in the case of the fourth figure mood AAI,  $p$ ), is not self-contradictory. For the validity of the proofs depends on the assumption that  $m$  (in the one case,  $p$ ) cannot imply both  $x$  and  $\bar{x}$ , an assumption which does not hold with self-contradictory concepts.<sup>1</sup> Such an assumption is not necessary with the first two groups, for their proof did not use this argument, but only arguments which would hold also even with self-contradictory propositions. For a self-contradictory proposition can imply something, and the relation of implication is still transitive. Also we can still argue from  $a \rightarrow b$  to  $\bar{b} \rightarrow \bar{a}$ , even where  $a$  is self-contradictory, otherwise we could not use a *reductio ad absurdum* argument.

I do not think that in these demonstrations I have assumed any logical axioms except the laws of identity (if that be a separate law), contradiction, and excluded middle. But it is interesting to note that the proofs of all three kinds of syllogisms presuppose the validity of the traditional first figure (with universal premisses) in the form—if  $s \rightarrow m$  and  $m \rightarrow p$ ,  $s \rightarrow p$ , thus justifying the old view of the priority of the first figure. This figure demonstrates the transitive character of implication, and if it were not

<sup>1</sup> I am indebted to Mr. W. Kneale for a comment on my paper which suggested this point. We have to make similar assumptions if we are to retain the square of opposition in its traditional form.

for the latter all the proofs would collapse. Some of the proofs also presuppose the principle that if  $p \rightarrow q$ ,  $\bar{q} \rightarrow \bar{p}$ . This need not be taken as self-evident any more than the principle that implication is transitive, but can be demonstrated as follows. Suppose we had a case,  $c$ , of  $p \bar{q}$ , i.e. a case in which  $p$  was true and  $q$  false for the same value of  $x$ . Then, since *ex hypothesi*  $p \rightarrow q$ , both  $q$  and  $\bar{q}$  would be true for the same value of  $x$ ,  $c$ , which is self-contradictory. So none but the most fundamental laws of thought are presupposed in my demonstrations. I do not think that even the commutative laws are, only the traditional three "laws of thought". The principle that, if  $p$  implies  $q$  and  $p$  is true,  $q$  is true, has sometimes been put forward as a separate independent axiom or postulate, but I do not see that it need be, for it is surely included in the meaning of "imply",<sup>1</sup> so that it cannot be denied without self-contradiction.

If the propositions in question are interpreted existentially, it is of course easy to make the requisite additions. With the universal proposition, all  $S$  is  $P$ , we simply add to the original non-existential interpretation the proposition "s is true for some values of  $x$ ." A particular proposition interpreted existentially becomes, as in the ordinary modern version, "s and  $p$  are both true for some values of  $x$ ". If these interpretations are adopted syllogisms can of course be treated in the classical way. In the Barbara mood all  $S$  is  $M$ , if there are cases of  $S$ , becomes "s is true of some values of  $x$  and  $s \rightarrow m$ "; from this and  $m \rightarrow p$ , it follows that  $s \rightarrow p$  and that  $p$  will be true of some actual values, i.e. at least those values of  $x$  of which  $s$  is true. We may note that it is here only necessary to assume that there are cases of  $S$ , the rest following. In the traditional third figure, however, it is necessary if we are to have a valid existential conclusion that  $M$  should be primarily given as existing also. This then will entail the existence of cases of  $s$  which will also be cases of  $p$ .

*Cambridge University.*

<sup>1</sup> Both in the Russellian and in my sense of "imply".

## A DISTINCTION BETWEEN SIMPLE AND COMPLEX QUALITIES

By PETER MINKUS

0.1. 'YELLOW' is sometimes said to name a simple quality, 'being a dog' is sometimes said to name a compound quality.

1.0. Let this be supported by saying that there are no marks by which we can decide whether a thing is yellow, whilst there are marks by which we can decide whether a thing is a dog. That a thing can bark, that it can growl, that it can wag its tail, that it likes gnawing bones, these and others are marks by which we decide it is a dog. There are no similar marks by which we decide that an apple is yellow.

2.0. Let this distinction be attacked as follows :—There are marks used in deciding whether a thing is yellow, just as there are marks used in deciding whether a thing is a dog. That a thing is gold-coloured is a mark of its being yellow, that a thing is lemon-coloured or fawn-coloured are further marks of its being yellow. So there are genuine marks of yellowness as much as there are genuine marks of being a dog.

2.1. There is something wrong with this objection. For clearly there are differences between the way we make use of the fact that a thing can bark when deciding whether it is a dog, and the way we make use of the fact that a thing is lemon coloured when we ask whether it is yellow.

2.11. Being lemon-coloured is too good a mark of being yellow to be called a genuine mark. For if a thing is lemon-coloured and we know it, we have a perfectly adequate reason for saying it is yellow. Yet, if a thing can bark and we know it, we do not have a perfectly adequate reason for saying it is a dog. For a thing might be able to bark and yet not be a dog. It might be a barking parrot or a ventriloquist at work. Admittedly, the fact that a thing can bark is a good reason and pretty good evidence for the conclusion that it is a dog, but the fact that a thing is lemon-coloured is much more ; it is not merely pretty good evidence that the thing is yellow, but an infallible guarantee of its being yellow. If you insist, by all means say that being lemon-coloured is a mark of being yellow, but in the name of consistency you should also say that being a St. Bernard

or being a dachshund are marks of being a dog. But this is quite absurd. For it is plain sense to say that a St. Bernard or a dachshund are sorts of dogs and not marks of dogs, which should suggest to you that it is plain sense to say lemon-colour and gold-colour are sorts of yellow and not its marks.

111. Some of the marks by which we establish that a thing is a dog, clearly, are more important than others. That an animal can wag its tail is a more reliable mark of its being a dog, than the fact that it has four legs. But the alleged marks of yellow could not differ in reliability. Gold-colour and lemon-colour as well as all the other shades of yellow are especially reliable signs of yellow. For they are equally superb and infallible signs of yellowness. When any one of them is present, no risk is taken in calling the object 'yellow'.

2.12. The various marks by which we decide whether a thing is a dog, do not exclude each other. A thing may bark and also be fond of gnawing bones, and it may at the same time be able to growl and snarl and have the other signs of being a dog. But could a thing be lemon-coloured and also gold-coloured and tawny and have the other alleged signs of yellowness? Clearly not. A given thing can have only one of these colours, just as a given thing cannot both be a St. Bernard and a dachshund. The various marks of a thing may come in clusters, the several instances of a class exclude each other.

1.121. A large group of the marks by which we decide whether a thing is a dog will be present in any dog. When we know that we are facing a dog, we may say that it can probably bark, that it probably likes to gnaw bones, etc. On the other hand, if we were sure that someone wore a yellow dress at the ball, none of us would be right in saying it was probably lemon-coloured. For the dress might, for all we would know, just as well have any other shade of yellow. The presence of a thing gives considerable probability to the presence of its marks, but the presence of a genus does not give considerable probability to the presence of all its species.

3.0. Are any properties related to yellow, as the property of gnawing bones with pleasure is related to being a dog?

3.1. Let someone say that there are such properties, namely all those shades of colour about which (1) we do not confidently say that they are yellow and of which (2) we are more inclined to say that they are yellow than that they are a different colour.

Being orange-coloured or being ochre-coloured are cases in point. Let it be said that these are marks of yellowness in that their presence does not guarantee the presence of yellowness and yet makes it probable, just as "gnawing bones with pleasure" does not guarantee "being a dog" and yet makes it probable.

3.11. The man who suggests this, is badly mistaken. For being ochre-coloured is no more a mark of yellowness than being a cross between a wolf and a dog is a mark of being a dog. The relation of ochre-coloured to yellow is like that of a doubtful instance to its class. It is different from the relation between "gnaw bones" and "is a dog" in two ways :

1. Given the fact that a thing is a dog, you rightly say it probably gnaws bones. But no one in his senses infers that a thing probably is ochre-coloured from the premiss that it is yellow.

2. If it is established that my shirt is ochre-coloured it may indeed be an open question whether it is yellow. But the question whether it is yellow cannot be settled by collecting further evidence concerning the shirt. It is settled by deciding whether we should give or not give a certain title to the ochre-colour of my shirt, namely the title "yellow". On the other hand, if it is established that there is a barking animal behind the tree, the question whether this animal is a dog or a barking hyena, is answered by collecting further evidence about the animal.

4.0. Is the property of emitting light rays of frequency  $L-L'$  related to "yellow" as the property of gnawing bones is related to being a dog ?

4.1. Suppose it were a fact that all dogs and only dogs have 28 vertebrae. Then let someone argue as follows :—The fact that an animal has 28 vertebrae is no doubt a mark by which we can decide it is a dog. In the same way the fact that an object emits light rays of frequency  $L-L'$  is a mark by which we can decide it is yellow. Hence, if the existence or availability of relevant marks makes a quality a compound quality, yellow is just as much a compound quality as being a dog. Certain frequencies of emitted light-rays, certain internal structures of surface pigments are marks by which we may establish yellowness.

4.11. There are marks and marks. There are marks by which we can, if we are good enough at zoology, distinguish a dog from a wolf, and there are marks by which we do, whether or not we are good at zoology, distinguish a dog from a wolf. To ignore the difference between marks and marks is a serious

source of trouble. People who ignore the difference between the most loosely connected and the most closely connected marks are apt to overlook the difference between Proper Names and Geometrical Concepts and they are apt to draw towards Internal Relations or the Doctrine of Pre-established Individual Histories. Against them we advertise that the several instances of the meta-concept "mark" have little in common.

4.12. There is a difference between the way in which certain marks are marks of being a dog, and the way in which other marks are marks of being a dog. There are marks of dogs about which a man must know something if he claims to know what a dog is. There is another kind of marks about which a man needn't know anything, even if he rightly claims to know what a dog is. A man may know what a dog is and he may have no idea of anatomy, physiology and zoology. A man may, therefore, know what a dog is and he may ignore that a dog has 28 vertebrae, 6 ribs, that it has no pancreatic gland, etc. But, is it at all clear that a man may know what a dog is, and yet ignore the fact that a dog has four legs, that it can bark, growl, snarl, likes to gnaw bones? It can be seen that as we accumulate these obvious and commonly used marks of dogs, we make it more and more absurd to say of a man who ignores all these marks, that he nevertheless knows what a dog is.

4.13. A man may surely know what yellow is, without having the least idea of optics and of the science, whichever it be, that deals with the structure of pigments. The fact that an object emits light rays of frequencies  $L-L'$  is a mark of yellowness, in the way in which having 28 vertebrae is a mark of being a dog, but IT IS NOT a mark in the way in which the ability to bark is a mark of being a dog. We might say it is a scientific mark of yellowness. But scientific marks of properties differ from the others in that they are not widely and commonly used in identifying the properties. Naturally this is reflected in the fact that they do not tend to be part of the current definition of the property.

4.2. It is objected that there is (1) no distinct (2) no lasting, difference between merely scientific and genuine marks of concepts. For (1) consider the fact that cows have four stomachs. Would you say with confidence that this fact constitutes a mere scientific associate of the concept of cowhood or that it constitutes a mark of cowhood? Would you take sides or would you uneasily avoid the issue? And (2) way back in history the fact

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that gold is heavier than other metals was not commonly used to identify gold. It was a scientific discovery known to and acted on by a narrow section of the public. But to-day the fact that gold is heavier than other metals is commonly used to identify gold. What was a mere scientific associate of the concept gold, has become a mark of gold. Hence it is quite imaginable that the mere scientific associates of the concept yellow, should one day be marks of the compound quality of yellowness.

4.21. But then it would no longer be yellowness in the sense we are familiar with. You cannot change the marks of a concept and maintain it is the same concept.

4.22. Is it a safe prediction to say that yellow will be commonly identified as the colour of an object which emits light rays of certain frequencies? It surely is a very unsafe prediction. Take the case of gold and its weighing more than other metals. This is the kind of fact that is easily and readily established by people. The technical equipment is: one balance. The routine is: simple use of a balance. The character of the fact that gold is heavier than other metals, invites popularity. It would have been safe to predict that this fact about gold would sooner or later be reflected in the definition of gold. Now consider the case of yellow and its characteristic frequencies. This is not a rule of nature which is easily and readily tested. The technical equipment consists of the better portion of a modern physical laboratory. The routine is one which it takes years of training to master. Is there any reason to suppose that this will alter, that in a century people will find it as easy to check frequencies as they now find it to check weights?

*Trinity College, Cambridge.*

## MORE ABOUT THE DENIAL OF SAMENESS OF MEANING

By PAUL WIENPAHL

IN an appraisal of Mr. Goodman's view that no two words ever have the same meaning Mr. Rollins has urged that either the claim is false or he has failed to understand it.<sup>1</sup> Since he has expended considerable effort to do so, the second alternative would imply that something has happened to the English language. An examination of this appraisal is in order because the appraisal enables us to throw additional light on this disturbingly "false" claim.

Furthermore, there is a line of analysis which, although quite different from Goodman's, leads to the conclusions to which Goodman's leads. The present writer has shown elsewhere that an examination of Frege's argument for the necessity for distinguishing between sense (meaning) and denotation (extension) implies, or seems to imply, that the sense (meaning) of a word is the combination of its (the token's) physical properties.<sup>2</sup> This clearly implies that no two words nor two occurrences of a word have the same meaning.<sup>3</sup> Thus it appears that, if Goodman's claims are false, so is this explication of meaning and, presumably, Frege's argument for the need for speaking of meaning (sense) at all—which is, so far as I know, the only precise demonstration for the need. This would seem in turn to indicate that either the falsity of Mr. Goodman's claim is curious or it is curiously important.

Mr. Rollins' remarks are divided into several parts: (a) two comments about the thesis that no two words ever have the same meaning; (b) similar comments on an extension of this thesis

<sup>1</sup> "On Likeness of Meaning," Nelson Goodman, *ANALYSIS*, Vol. 10, pp. 1-7.

<sup>2</sup> "The Philosophical Denial of Sameness of Meaning," C. D. Rollins, *ANALYSIS*, Vol. 11, pp. 38-45.

<sup>3</sup> "Frege's *Sinn und Bedeutung*," P. Wienpahl, *Mind*, Oct., 1950, pp. 483-495.

<sup>4</sup> In correspondence Mr. Richard Rudner has indicated to me that this explication of 'sense' violates a condition of Frege's argument upon which it is supposedly based. The explication requires that no two occurrences of a word have the same meaning (sense) yet the cogency of Frege's demonstration requires that we can say that two occurrences of a word do have the same meaning (sense). This suggests that we should say that they have similar not the same sense. This shows, it seems to me, that we are getting at an analysis of 'same' as well as 'sense'. Hence, it is worth noting that in the sentence in the body of this paper from which this footnote comes could read either "... not two occurrences of the same word . . ." or "... nor two occurrences of a word . . ." 'Same' is frequently unnecessary and this makes its analysis confusing and difficult. Generally, the fact that a given word is sometimes unnecessary and could be omitted may be a source of the belief that meanings are subsistent entities.

which holds that even two occurrences of the same word do not have the same meaning ;<sup>1</sup> (c) an examination of the acceptability of the argument which Mr. Goodman employs to lead up to his thesis ; (d) a demonstration that the criterion of sameness of meaning in which this argument issues produces " wild results " ; and (e) a statement of the possible causal sources of Mr. Goodman's " puzzling thesis ". I shall address myself only to (a) and (d) directly and (b) by implication, since they bear particularly on the view that meaning (sense) is the physical property of a sign. In the examination of these points I am not concerned to show that they are false. I am interested rather in showing that they are not well-taken and that they, therefore, obscure both the significance of Mr. Goodman's thesis and an important aspect of philosophic analysis.

Mr. Rollins, then, urges that Mr. Goodman's thesis, if he correctly understands it, cannot possibly be true (p. 39). He thereby overlooks the possibility which Mr. John Wisdom has shown us that philosophic statements may be false but illuminating. I shall develop this point by examination of the difficulties which Mr. Rollins finds.

The claim that no two words ever have the same meaning cannot be true in the first place, writes Mr. Rollins, because " we often say, understand, agree, prove and disprove such a thing as that two different words have the same meaning ". (p. 39). Now it must be noticed that we *do* say, etc. this. However, we could be wrong. Thus, to point this out does not demonstrate the falsity of Mr. Goodman's claim. More importantly, however, we might *use* ' same ' in the phrase ' same meaning ' correctly without knowing what it *means*. Mr. Goodman's claims may help us to clarify the meaning of ' same ' in its occurrences in phrases like ' same meaning ' and this clarification, although it may eventually affect usage, cannot be disregarded on the grounds that it may. The supposition that it can issues, I believe, from the view that words have a necessary meaning which is an eternal essence. This view makes us use language sceptically and see truth or falsity in everything which another says instead of illumination.

Mr. Rollins goes on to urge that if we cannot say truly that some words do have the same meaning it is nonsense to say that they never do. This because ' same meaning ' and ' not same meaning ' " work and apply and have sense only as opposites " (p. 39). Unless I misunderstand Mr. Rollins here,

<sup>1</sup> "A Note on Likeness of meaning," Richard Rudner, ANALYSIS, Vol. 10, 1950, pp. 115-118.

he must be able to demonstrate that to say "No two words ever have the same meaning" is different from saying "No two men ever have the same fingerprints". In one way, of course, it is quite obviously different. In the way in which Mr. Rollins apparently intends, it is not. For example, it is difficult to see how two men can have the same fingerprints without being the same man. In any case, to maintain the difference requires explication both of 'same' and 'meaning'. Therefore, the point at issue here seems to beg the question because Mr. Goodman's claims bear upon explicating 'same' and mine bear upon explicating 'meaning' ('sense'). The results of this work indicate that when we do say two words have the same meaning we speak loosely. To claim that these results are false requires a different explication of 'same' and 'meaning'.<sup>1</sup>

Mr. Rollins then considers the possibility that Mr. Goodman's thesis makes sense on the grounds that to deny sameness of meaning "amounts only to saying that the respective workings of two different words are *not so much alike as the respective workings of two occurrences of the same word*"; so that 'same meaning' acquires sense (and truth) from the latter kind of case but becomes false in the former. However, he urges, this will not do because we often do say, understand, etc., that two different words have the same meaning. Furthermore, would it not create confusion to use the expression 'similar meaning' in place of the expression 'same meaning'? It would obscure the fact that normally when we say that two words have the same meaning we are saying of them what we can say of two occurrences of the same word (p. 40).

Here again there is an important way in which Mr. Rollins is beside the issue. We can grant what he says without changing Mr. Goodman's conclusions. Furthermore, in concentrating on the truth or falsity of these conclusions, he obscures the fact that if we take Mr. Goodman's work to be a clarification of 'same meaning' we can continue to use the phrase in the places in which Mr. Rollins urges that we do and should but be clearer about its use. Finally, it may be observed that 'meaning' is ambiguous. It is used both for 'sense' and 'denotation'. Thus, regardless of Mr. Goodman's claims it will often appear and in fact be obvious that we *can* say that two different words have the same meaning. This will occur when 'meaning' is

<sup>1</sup> This begging the question suggests that Mr. Rollins is not clear about the distinction between use and meaning although he refers to G. E. Moore's distinction between "Do we know how to use expression 'X'?" and "Do we know how to describe our use of it?" (p. 45).

used for 'denotation' which is very likely its most common use when it appears in 'the same meaning'.<sup>1</sup>

Mr. Rollins continues by pointing out *inter alia* that, even if we ignore the inacceptability of Mr. Goodman's argument which leads to his "puzzling thesis" (p. 42-3), we must see that the criterion of sameness of meaning on which it is based is unsatisfactory. The ground on which it is unsatisfactory is that it gives "wild results" '(and, therefore, presumably conflicts with the facts of ordinary usage) (pp. 43-44).

This criterion is that we may judge that 'triangle' and 'trilateral' differ in meaning because, although having the same extension, "there can be found for them at least one pair of similarly formed compounds, for example, a) 'triangle-description' and b) 'trilateral-description', which do differ in extension since 'triangle' is included in the extension of (a) but not in that of (b)"; and similarly for 'trilateral'. (p. 43). But this leads to such wild results as that 'triangle' and 'TRIANGLE' differ in meaning and that 'centaur' at a certain place in your copy of *Analysis* does not have the same meaning as 'centaur' in the same place in my copy.<sup>2</sup>

The basis of this objection is again that of conflict with ordinary usage. And again we must observe that the criterion may lead to results which conflict with the facts of ordinary usage and be itself nonetheless useful and/or correct. Even if we agree with Mr. Rollins that the results conflict with the facts of ordinary usage, we do not have to accept the conclusion that the criterion is unsatisfactory any more than we have to conclude that the thesis to which the criterion leads is false because it is "puzzling" or violates ordinary usage.<sup>3</sup>

So far I have simply pointed out that the thesis first and the criterion second can be interpreted as clarifications if we do not stress their truth or falsity. I now wish to urge that neither Mr. Goodman's thesis nor his criterion do in fact conflict with the facts of ordinary usage. They clarify them. We can still say ordinarily that two words have the same meaning, but we shall

<sup>1</sup> When 'meaning' is used for 'denotation' there are also cases in which 'same' need not be used. For example, instead of saying "X and Y have the same denotation" we can say "X has the denotation which Y has" or "X has the denotation and only the denotation which Y has".

<sup>2</sup> Notice that instead of saying "at the same place in my copy" we could say "at that place in my copy", or the whole clause could read "that 'centaur' on page x in your copy of ANALYSIS does not have the same meaning as 'centaur' on page x in my copy."

<sup>3</sup> Furthermore: (a) confusing 'meaning' as 'sense' with 'meaning' as 'denotation' would make these results seem wild, and (b) this objection also begs the question because it assumes that we know what 'same' and 'meaning' mean (*sic*) which is the point at issue.

be clearer about it when we do so in philosophy where it is important.<sup>1</sup>

Not only can this work be interpreted in this way, it should be. Not to do so is to misunderstand philosophy and derives, it seems to me from a greater interest in truth and falsity than illumination. This misses the *raison d'être* of philosophic analysis and confuses science with philosophy. It is a tendency which has made meanings mysterious by making them a kind of thing, albeit a funny kind. This requires the category of subsistent being which Mr. Rollins does not like either (p. 45).

*Santa Barbara College.  
University of California.*

<sup>1</sup> By being clearer about it I have in mind, for example, being able to work on logical problems without employing the fuzzy notion of subsistent entities and without confusing meaning as 'sense' and meaning as 'denotation'.

## THE THEORY OF TYPES—A FURTHER NOTE

By J. J. C. SMART

SINCE sending off a rejoinder<sup>1</sup> to Mr. Shearn's reply<sup>2</sup> to my original article on "Whitehead and Russell's Theory of Types"<sup>3</sup> I have now come to the conclusion that I was grossly unfair to Mr. Shearn. I now think that Mr. Shearn has done a great deal to illuminate the main issue; he has certainly been instrumental in illuminating my own mind. I now see that my original criticisms do not apply to the *Second* edition of P.M. It is certainly true that in the introduction to the second edition of P.M. the authors *do* regard " $f(\phi, \psi, x, y)$ " for example, as a truth function (using this term in an extended sense rather similar to Ramsey's) of elementary propositions. My criticisms do not in the least apply to P.M. as modified on these lines. (Of course fresh criticisms now become possible, namely those questioning the thesis that mathematical propositions can all be regarded as truth functions of elementary propositions).

I also thoroughly agree that what I originally said had no special relevance to the ramified part of the theory of types. About this I should now want to say some of the things that Ramsey said.

The main burden of my two articles, that there is nothing mysterious or unnatural about type-restrictions, when these are properly stated, I still regard as valid. I hope that this truth will not be lost amid the errors from which, as I now gratefully acknowledge, Mr. Shearn has delivered me.

*University of Adelaide, Australia.*

<sup>1</sup> ANALYSIS, Vol. 11, No. 6.

<sup>2</sup> ANALYSIS, Vol. 10, No. 4.

<sup>3</sup> ANALYSIS, Vol. 11, No. 2.

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